winters at Cincinnati, Ohio, 1788–1813, and 1817–1835 (when reliable instrumental records began), kindly compiled for me by Mr. W. C. Devereaux, the period of winters from 1800–1808 is similar to the New Haven record. (See fig. 1.) Figure 1 shows also the sequence of temperature (relative to average of last 50 years) from 1868 or 1870 to 1888 and from 1913 to 1921 at New Bedford, Mass., New York, N. Y., Washington, D. C., Cincinnati, Ohio, and Chicago, Ill. It will be noted that the sequence of the few winters preceding the beginning of the strong alternations was essentially the same in the series of 45 years ago and 113 years ago as that now in progress, and that the swings up and down are of the same order in the three periods. It seems not unreasonable to expect that the winter of 1921–22 will be a cold one and possibly that that of 1922–23 will be a warm one. Before we should dignify such expectations with the term "forecast," however, the characteristics of the weather in the early eighties, and, if possible, in the first decade of last century, should be compared closely with those of the present time, to enable us to recognize whether or not our present weather has characteristics of the years immediately preceding the break-up of the earlier periods of alternating winters.

Perhaps by finding the common factors of the general weather of North America and of the North Atlantic in the periods 1870–1876 and 1915–1921 we can get a preliminary understanding of why the winters alternate, and by studying the general weather of 1876–1883 what indications in the near future may be recognized as presaging the end of our current period of alternations.

Even if we can not say for winter after winter what the character is likely to be, we can say that immediately after a cold winter the chances are two to one or better in favor of a mild or warm one, and that a period of alternating cold and warm winters which is general over a large part of the eastern United States may continue for several winters, as cold-warm-cold, etc.

CONCLUSION.

Our winter temperature data show that in the sequence of winters are evidences of some control, and therefore that studies of the positions of grand centers of action of the atmosphere and their changes from winter to winter are well worth undertaking if we would have successful forecasts of the character of winters.

DISCUSSION.

By H. W. CLOUGH.

The paper of Dr. Brooks is essentially a contribution to the question of the existence of an approximate two-year period in weather. Such a period has been claimed by other investigators, notably Clayton [1] and Helland-Hansen & Nansen.[2] Clayton investigated a 25-month period which he found to persist with remarkable regularity during the seventies and eighties of the last century, but later on the periodicity disappeared.

Obviously it is important to determine to what extent these alternations differ in amount from what would be expected if there were no relationship between one winter and the following one. Besson has shown that in a series of N numbers there are $\frac{5}{12}$ (N–3) single rises and falls, or about 41 for every 100 numbers. This is the total number of single rises and falls. It is necessary in addition to determine the relative frequency of groups of successive alternations from 1 to 10. The formula deduced by the writer giving the probability of a series of n successive rises and falls in a series of unrelated numbers is approximately $\left(\frac{5}{12}\right)\left(\frac{3}{8}\right)^2\left(\frac{5}{8}\right)^{n-1}$. Thus the probability of a series of 10 successive rises and falls is about 0.085 for every 100 numbers and the total number of groups of five or more successive rises and falls is about 1.5 for each 100 numbers.

Applying Besson's tests to some of the series of winter temperatures compiled by Dr. Brooks, the results are shown in the following table:

Stations.	Number of years record.	Number of crests and hollows.	Per cent.	Single rises and falls.	Theoretical number.
Chicago	90	62	69	45	37
Cincinnati	85	57	67	40	34
Baltimore	103	70	68	45	42
New Bedford	107	68	64	43	43

It will be seen that the number of crests and hollows averages close to the theoretical 67 per cent for each 100 numbers. The number of single rises and falls is somewhat greater than the theoretical number for Chicago and Cincinnati and about the same for the longer series at New Bedford and Baltimore. The excessively large number of alternations in the seventies and eighties probably accounts for the excess in the number of single rises and falls. This is a unique series of 12 alternations and the chances are for one such occurrence in about 3,000 years if there were no relation between successive winters. This sequence, however, is so unique that it may be doubted whether it would occur again in 500 years or more. There is nothing even remotely paralleling it during the 140 years since observations are available in the United States. It is hard to escape the conclusion that this series indicates some sort of systematic relation between the successive winters, but, on the other hand, it may be equally true that the particular grouping of events combining to produce this series might not again occur in centuries.

There have been other groups of five or more successive alternations at single stations during the past 140 years. At Chicago there were two such groups of five or more single rises and falls; at Cincinnati, 3; at New Bedford, 2; at Baltimore, 2. The only noteworthy series common to two or more stations was one from 1890 to 1899, comprising seven to eight successive alternations shown at New Bedford, Baltimore, and Cincinnati. Some of the changes were, however, less than 2°, and changes from warmer to colder or colder to warmer, instead of changing from one side of the normal to the other side. The series shown on the diagram (p. 72) in Dr. Brooks's paper, from 1804 to 1810 at Cincinnati and New Haven, contains five alternations.

There are therefore, two, or at most three, series of five or more alternations during the past 140 years, covering any extensive area. The theoretical number, as stated above, is about 2. There is little, therefore, in this showing to support the theory of a systematic tendency to alternations of winter temperatures. The observed deviations from the theoretical number for a series of

[1] Clayton, H. H., A lately discovered meteorological cycle. *Amer. Metl. Jour.*, vol. 1 1885, pp. 130, 538.

[2] Helland-Hansen & Nansen, Temperature variations in the North Atlantic Ocean and the atmosphere. Smithsonian Institution. Misc. Coll., vol. 70, No. 4, 1920, p. 262.

unrelated numbers may be plausibly regarded as merely the probable deviations of random sampling.

Another line of evidence tending to show the fortuitous character of the occurrences relates to the probability of the frequency of the intervals from crest to crest or hollow to hollow in a series of unrelated numbers. This serves as a criterion for testing the conformity of meteorological data to the requirements of fortuitous occurrence. According to Besson, in a series of unrelated numbers the number of two-intervals is greater than the number of three-intervals in the ratio of 40 to 33. A large number of series of annual means of temperature and pressure at stations in Europe and the United States were examined and practically all show a decided preponderance of the three-year interval.

Dr. Brooks's later conclusions, therefore, that the sequences of winter temperatures, particularly as regards alternations of warm and cold, are mainly what would be expected from chance occurrence seems to be borne out by the evidence presented above.

A POSSIBLE RAINFALL PERIOD EQUAL TO ONE-NINTH THE SUN-SPOT PERIOD.

551.578.1 : 551.590.2

By DINSMORE ALTER, Ph. D.

[University of Kansas, Lawrence, Kans., Oct. 1, 1920.]

SYNOPSIS.

The first search for a rainfall periodicity was based on a hypothetical constant periodicity equal to Turner's earthquake period which is exactly one-ninth of the mean sun-spot period. The results were inconclusive, but indicated that a further search might be worth while.

Next a graphical method was devised by which it was possible to vary the length of this periodicity, keeping it always in step as the ninth harmonic of the varying sun-spot period. The results obtained were much more conclusive.

All the sectional rainfall averages for each of the 42 sections of the United States were examined and tables and curves showing the results for each of three parts of the United States (Eastern, Central, and Pacific) are appended to the paper. The first half and the last half of the data were for the most part considered separately and a fair similarity obtained from these stretches of independent data. Curves from these separate data are plotted together in order to show plainly points of resemblance and of dissimilarity. The reader must judge for himself whether these resemblances can be accidental or must be due to a real periodicity.

The work is being continued and it is proposed to examine the data of other countries and to search for other possible harmonics of the sun-spot period. An examination of temperature data is also planned.

INTRODUCTORY.

In August, 1915, Dr. A. E. Douglass read a very interesting paper before the Berkeley meeting of the American Astronomical Society regarding an investigation of the growth of trees in many parts of the world, indicating an 11-year period in rainfall (1).

It seemed to me that the data collected by the Weather Bureau should definitely settle such a question of periods. Some preliminary reading showed, however, that a tremendous amount of time had been spent on the problem (2) and that if solvable it must be very complicated. Other work prevented starting any actual investigation; then the war intervened and the problem was untouched till the spring of 1919. The first data examined were those from Lawrence, Kans., where records since 1868 are available. Several hundred hours of work showed nothing. Once a stretch of five years was found which resembled another five quite closely after eliminating the seasonal curve. Another time resemblances were found after about 22 years. All such were easily explainable as accidental. It seemed useless to carry the work further with the data at hand.

A paper by Prof. Turner (3), however, gave me a new suggestion, although there was little if any logical reason for any connection. In this paper, Prof. Turner shows plainly the existence of a period in earthquakes with a length between 14.8421 and 14.8448 months. It occurred to me that this period might be commensurable with the sun-spot period. Upon multiplying it by 9 I obtained 11.13 years, which is the mean sun-spot period to the exact hundredth of a year. Such an exact coincidence is very probably not accidental (4).

My next move was to examine all sun-spot data in order to find whether such a period also exists in sun-spots as the ninth harmonic of the primary period. The preliminary results based on a constant period equalling one-ninth the mean sun-spot period were inconclusive though possibly favoring the existence of the period. After the investigation of the rainfall data the problem was attacked again in a different manner and led to a much more positive result. This will be discussed more fully in a later paper. The idea now came to me to investigate the rainfall data by the same method that Prof. Turner has used on the earthquake data. This method is available only after some extraneous idea has indicated at least an approximate period to be investigated.

MATERIAL SUITABLE FOR HARMONIC ANALYSIS.

A mass of observational material when plotted with time as abscissae and observed values as ordinates may show no repetition of the same curve, even though such a curve might exist. There may be nothing definite about it to indicate a period. In such cases ordinary methods of harmonic analysis become useless. This failure to repeat values, when a period exists, may be due to any one or more of the four following causes:

(*a*) Incommensurable periods may coexist. In this case the curve will never repeat itself, although for short periods of time there may be a fairly close approximation to such repetition. If there are three or more incommensurable periods the curve obtained for the data is very complex. For example, the seasonal variation of the rainfall would be incommensurable with a possible one equalling the sun-spot period. Of course, if one of such periods is known, as in the case of the seasonal variation in the example above, it may be eliminated.

(*b*) There may be large accidental errors. Such errors mask a periodicity almost completely in any one cycle and disappear only when the data values in each of a number of well-distributed phases are added through many cycles. From the theory of errors their influence will be inversely proportional to the square root of the number of cycles added.

(*c*) Long-period variations may exist. If there are periods longer than the interval of the data they will produce much the same effect as accidental errors or incommensurable periods.

(*d*) There may be periods which vary in length. An example of such a period is the sun-spot period, which, although averaging 11.13 years, has varied from 7.3 to 17.1 years during the last 115 years.